# PROCEEDINGS

OF

# THE ROYAL SOCIETY.

1832–1833. No. 12.

February 21, 1833.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

A paper was read, entitled, "On the Influence of the Sun's Rays on the Oscillations of the Magnetic Needle." By William Snow Harris, Esq. F.R.S. In a letter addressed to Samuel Hunter Christie, Esq. M.A. F.R.S.

The apparatus employed by the author in the inquiries of which he gives the results in the present paper, is very similar to that he has already described in his former communications to the Royal Society. It allowed of his carrying on a long series of experiments with freely suspended magnets, oscillating in a medium either rare or dense, and either in the sunshine or in the shade. The source of error incident to experiments in sunshine, made under an air-tight receiver, arise from the increased temperature, producing, both in the rare and in the dense medium, an irregular expansion, and a constant circulation of currents of air, which interfere with the equable movements of the bar—a condensation of vapour on the interior of the receiver—an expansion of the bar itself, by which its length, as a pendulum, becomes changed—and, lastly, a derangement of the original magnetic state of the bar. These disturbing causes he endeavoured to avoid by observing the oscillations, first in the shade, under a close receiver, and next when a beam of sunshine was thrown into the receiver by means of a plane mirror; in which case the heat was inconsiderable. When the bar had been allowed to return to its former temperature, similar experiments were repeated, after exhausting the receiver. The results of a series of experiments conducted in this manner are given in several tables: and the author concludes from them that the influence of the solar rays on a magnetic bar, oscillating in air, is to increase its apparent rate of oscillation; while in vacuo, that rate is diminished.

The author seeks for an explanation of these phenomena in certain changes effected in the surrounding medium. Comparative experiments were instituted on a bar of copper of the same dimensions as the magnetic bar employed in the former series. The author concludes from these inquiries, that the phenomena in question are independent both of the magnetic state of the bar, and also of the influence of solar light. He tried the effect of exposure of the bar to the intense light evolved by lime, acted upon by the influence of the oxy-hydrogen blowpipe; but with the same negative result.

An Appendix to the preceding paper was also read, entitled, "Remarks on Mr. Snow Harris's Communication," by S. H. Christie, Esq. M.A. F.R.S.; in which the latter gentleman, although he admits that Mr. Harris's experiments may explain some of the phenomena observed by Mr. Christie, yet he does not consider them as conclusive against the existence of the magnetic influence of the solar rays, and points out several circumstances in support of that opinion.

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February 28, 1833.

MARK ISAMBARD BRUNEL, Esq., Vice-President, in the Chair.

A paper was read, entitled, "A Relation of the case of Thomas Hardy Kirman, with remarks on Corpulence." By Thomas Joseph Pettigrew, Esq. F.R.S.

The subject of this communication, T. H. Kirman, was born at Barrow Mill, near Barton-upon-Humber, in Lincolnshire, on the 18th of April 1821. His father, who is a miller, is of middle stature, but inclined to corpulency; his mother tall and stout; and both perfectly healthy. Their son Thomas was not remarkable at the time of his birth for any peculiarity either in size or strength. He has enjoyed uniform health, excepting that when six years old he fractured his thigh, and was in consequence confined for five weeks to his bed; on rising from which, by an imprudent exertion, he injured his knee, and was obliged to lie upon a couch for five or six weeks longer. It was during this period of inactivity that he was first observed to increase much, both in bulk and height. This increase has since been progressive; and especially rapid during the last twelve months. At the present time, at which he is within two months of being twelve years old, he measures five feet one inch in height, and weighs one hundred and ninety-eight pounds. He measures round the chest $45\frac{1}{2}$ inches, round the abdomen 44 inches, round the pelvis $48\frac{1}{2}$ inches, round the thigh 27 inches, round the calf of the leg $18\frac{1}{2}$ inches, round the upper arm 13 inches, round the fore arm $11\frac{1}{2}$ inches, round the wrist 7 inches, and across the shoulders 19 inches.

The fat deposited is of firm consistence, and the muscular frame is strongly developed. His size occasions him at present but little inconvenience; his appetite and sleep are moderate; his habits and sports perfectly juvenile; and there is no appearance of puberty. He has a brother and two sisters, who are all of the ordinary height and size.

This account is followed by remarks on the subject of corpulency. The author observes, that this habit of body is most frequently met with in marshy districts, and has an apparent relation with the humidity of the climate. It is much more prevalent in England than in France or the South of Europe. It may often be traced to hereditary predisposition, and is promoted by tranquillity and cheerfulness of mind, and equability of temper, by full living, the use of fermented liquors, and of certain articles of diet containing much nutritious

matter, and also by inaction of body, whether the result of natural indolence, or, as was originally the case in the present instance, of necessary confinement.

Various other circumstances are enumerated by the author as favouring the accumulation of fat; and various expedients pointed out for obviating this morbid tendency, founded on the principles of diminishing the supply of nutriment, of increasing the tone of the system, and stimulating it to greater activity.

The reading of a paper, entitled, "Experimental Determination of the Laws of Magneto-electric Induction in different masses of the same Metal, and of its Intensity in different Metals," by Samuel Hunter Christie, Esq. M.A. F.R.S., was commenced.

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March 7, 1833.

JOHN WILLIAM LUBBOCK, Esq. M.A., V.P. and Treasurer, in the Chair.

The reading of Mr. Christie's paper was resumed and concluded.

Mr. Faraday, in his valuable papers entitled "Experimental Researches in Electricity," has advanced the proposition, that "when metals of different kinds are equally subject, in every circumstance, to magneto-electric induction, they exhibit exactly equal powers with respect to the currents which either are formed or tend to form in them;" and "that the same is probably the case in all other substances." The author not being satisfied with the conclusiveness of the experiments adduced in support of this proposition,—in order to determine its correctness, subjected different metals directly to the same degree of magneto-electric excitation, in such a manner, that the currents excited in them should be in opposite directions (as was the case in Mr. Faraday's experiment), and also that these opposing currents should have the same facility of transmission; so that the difference of their intensities, if any existed, might admit of measurement. He then minutely describes the apparatus he contrived with this view, and which consisted of helices of copper and of iron wire, covered with silk, each making sixty-five turns, but in opposite directions, and crossing each other alternately, and surrounding a cylinder of soft iron, which was rendered magnetic by the application of the large magnet belonging to the Royal Society, which the Council had placed at his disposal while engaged in these researches. The result of the experiment showed that the force of the currents from the copper helix considerably exceeded that from the iron helix, and appeared to be even more than double. By a modification of the apparatus, he found that the intensities of the currents in the two wires were very accurately proportional to their conducting powers; and hence the uniformity of the results obtained by Mr. Faraday is easily explicable.

The next object of Mr. Christie was to determine the order of the

relative intensities and conducting power of several of the metals: but previously to engaging in this inquiry, he made a series of experiments, with a view of determining the law of intensities as depending upon the length and diameter of the wire through which the current is transmitted. For this purpose it was necessary to devise means of making and breaking the contact in as invariable a manner as possible. This he accomplished by letting the same weight fall from a constant height when the contact was to be broken, and suddenly relieving the cylinder of the tension caused by the same weight when the contact was to be formed. He ascertained that portions of wire connected with the one which formed the circuit, but not included in the circuit itself, had scarcely any influence on the intensity of the current. He then enters into various theoretical investigations as to the mode of deducing the absolute intensities of the currents in this mode of experimenting.

By comparing the intensity of the electricity in wires of one metal with that in wires of each of the others, by means of the arrangement described in the beginning of the paper, and taking a mean of the results, he found the relative intensities in the following metals to be, silver 1520, gold 1106, copper 1000, zinc 522, tin 253, platinum 240, iron 223, and lead 124. The author compared these results with those obtained by Davy, Becquerel, Professor Cumming, and Mr. Harris, and states what he considers may have been the causes of the differences.

The second object of the author's inquiry relates to the law of variation of the intensity of the electricity excited in wires of different diameters: for determining which he compares the effects of three different wires of which the diameters were in the proportion of 4, 2, and 1. The results occupy several tables: and the deduction from them, with regard to the law in question, is, that the intensity varies nearly as the square of the diameter: but several causes contribute to interfere with the accuracy of this determination, and to exhibit the power as a mean of 1·844 instead of 2; the principal of which is the action of the coils upon each other.

By other methods, in which two wires of different lengths and diameters are placed so as to oppose each other in their effects, the accuracy of the conclusion that this power is the square, was satisfactorily established. Hence he arrives at the general conclusion, that the intensity or conducting power varies as the mass or weight directly, and as the square of the length inversely.

A paper was then read, entitled, "Note on the Tides." By John William Lubbock, Esq. V.P. and Treasurer of the Royal Society.

This communication contains some interesting results which Mr. Lubbock has obtained from observations made at Plymouth, Portsmouth, and Sheerness, under the superintendence of the Masters attendant at those dockyards. Mr. Dessiou has, with extraordinary perseverance, just completed the discussion of about 6000 additional observations of the tides at the London Docks, with a view to found on a more certain basis the corrections of the moon's parallax and

declination. The results which he has obtained are utterly irreconcilable with the theory of Bernoulli, and therefore the tables computed upon that theory must be rejected as inaccurate.

A paper was also read, entitled, "On the Nature of Sleep." By A. P. W. Philip, M.D. F.R.S. L. & E.

The author intends the present paper as a continuation of his inquiries into the relations subsisting between the nervous and muscular systems, which form the subject of his former papers, but which would be incomplete without the consideration of their condition during sleep. With this view he proposes to determine the particular organs, on the condition of which this peculiar state of the system depends; the laws by which it is governed; and the influence it has upon other parts of the system. The necessity of intervals of repose applies only to those functions which are the medium of intercourse with the external world, and which are not directly concerned in the maintenance of life. The organs subservient to these two classes of functions may be viewed as in a great degree distinct from one another. The brain and spinal marrow constitute alone the active portions of the nervous system. The law of excitement, which regulates the parts connected with the sensorial functions, including sensation, volition, and other intellectual operations, and the actions of the voluntary muscles, is uniform excitement, followed by a proportional exhaustion; which, when occurring in such a degree as to suspend their usual functions, constitutes sleep; all degrees of exhaustion which do not extend beyond the parts connected with the sensorial functions being consistent with health. On the other hand, the law of excitement of those parts of the brain and spinal marrow which are associated with the vital nerves, and are subservient to the vital functions, is also uniform excitement; but it is only when this excitement is excessive that it is followed by any exhaustion; and no degree of this exhaustion is consistent with health. The law of excitement of the muscular fibre, with which both the vital and sensitive parts of the brain and spinal marrow are associated, namely, the muscles of respiration, is interrupted excitement, which, like the excitement of the vital parts of these organs, is, only when excessive, followed by any degree of exhaustion. The author conceives that the nature of the muscular fibre is everywhere the same; the apparent differences in the nature of the muscles of voluntary and involuntary motion depending on the differences of their functions, and on the circumstances in which they are placed: and he concludes, that, during sleep, the vital, partaking in no degree of the exhaustion of the sensitive system, appears to do so simply in consequence of the influence of the latter on the function of respiration, the only vital function in which these systems co-operate.

The author proceeds to make some observations on the cause of dreaming, the phenomena of which he conceives to be a natural consequence of the preceding proposition. In ordinary sleep, the sensitive parts of the brain, with which the powers of the mind are associated, are not in a state of such complete exhaustion as to preclude their being excited by slight causes of irritation, such as those which

accompany the internal processes going on in the system. The sensorium is the more sensible to the impressions made by these internal causes, inasmuch as all the avenues to external impressions are closed, and the mind is deprived of the control it exercises, during its waking hours, over the train of its thoughts, by the help of the perceptions derived from the senses, and the employment of words for detaining its ideas, and rendering them objects of steady attention, and subjects of comparison.

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March 14, 1833.

The Rev. WILLIAM BUCKLAND, D.D., Vice-President, in the Chair.

A paper was read, entitled, "On the Figures obtained by strewing Sand on Vibrating Surfaces, commonly called Acoustic Figures." By Charles Wheatstone, Esq. Communicated by Michael Faraday, Esq. D.C.L. F.R.S.

The author, after adverting to the imperfect notice taken by Galileo and by Hooke of the phenomena which form the subject of this paper, ascribes to Chladni exclusively the merit of the discovery of the symmetrical figures exhibited by plates of regular form when made to sound. He proposes a notation, by means of two numbers separated by a vertical line, for expressing the figures resulting from the vibrations of square or rectangular plates. He gives a table of the relative sounds expressed both by their musical names and by the number of their vibrations, of all the modes of vibration of a square plate, as ascertained by the experiments of Chladni. He then proceeds to class and analyse the various phenomena observed under these circumstances, and shows that all the figures of these vibrating surfaces are the resultants of very simple modes of oscillation, occurring isochronously, and superposed upon one another; the resultant figure varying with the component modes of the vibration, the number of the superpositions, and the angles at which they are superposed. In the present paper, which forms the first part of his investigation, he confines himself to the figures of square and other rectangular plates.

The author finds that the principal results of the superposition of two similar modes of vibration are the following:—first, the points where the quiescent lines of each figure intersect each other remain quiescent points in the resultant figure; secondly, the quiescent lines of one figure are obliterated, when superposed, by the vibratory parts of the other; thirdly, new quiescent parts, which may be called points of compensation, are formed whenever the vibrations in opposite directions neutralize each other; and, lastly, at other points, the motion is as the sum of the concurring, or the differences of the opposing vibrations at these points. After considering various modes of binary superposition, the author examines the cases of four co-existing superpositions.

When the vibrations of the superposed modes are unequal in intensity, there is formed a figure intermediate between the perfect re-

sultant and one of its compounds. These figures the author denominates *imperfect resultants*.

In each series of transitions there are certain points which are invariable during all the changes: these are quiescent points, formed by the nodal lines of one figure intersecting those of the other, and the centres of vibration, where the maxima of positive or negative vibration agree in each component mode of vibration. The points of compensation are changeable. Transitional figures appear when the sides of the plate are nearly, but not exactly, equal.

The author next considers the figures obtained on square plates of wood and other substances, having different degrees of elasticity in different directions. He concludes this part of his paper by an account of some optical means of representing the figures noticed by Chladni.

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March 21, 1833.

WILLIAM GEORGE MATON, M.D., Vice-President, in the Chair.

A paper was read, entitled, "An Account of two cases of inflammatory Tumour produced by a deposit of the Larva of a large Fly (*Œstrus humanus*) beneath the Cutis in the Human Subject; accompanied with Drawings of the Larva." By John Howship, Esq. Communicated by Charles Hatchett, Esq. F.R.S.

The first of these two cases is that of a soldier stationed on the banks of the Marawina river in Surinam, who had a large boil on the back, from which a maggot was pressed out. The second case, which occurred at Santa Anna, in the district of Maraquita, in Columbia, is that of a carpenter, who had for some months a large boil on the scrotum, from which a living larva was extracted. A description of this larva, drawn up by Mr. Curtis, is given by the author, together with a drawing of the specimen. The author proposes giving to it the name of the *Œstrus humanus*.

The reading of a paper, entitled, "Experimental Researches in Electro-magnetism," by the Rev. William Ritchie, LL.D. F.R.S. was commenced.

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March 28, 1833.

The Rev. JAMES CUMMING, M.A., Vice-President, in the Chair.

The reading of Dr. Ritchie's paper was resumed and concluded.

This communication consists of three parts. In the first part the author shows that the common deflecting galvanometer, in which the deflecting forces are assumed to be as the tangents of deflection, is founded on false principles, and consequently leads to erroneous results. The wire forming the coil is of considerable thickness, and therefore there is no fixed zero from which the deflections can be reckoned. The length of the coil, also, being generally short, occa-

sions another serious error, as the theoretical investigation is founded on the supposition of an indefinite length. In proof of the inaccuracy of the indications of the common deflecting galvanometer, the author took two elementary batteries, the plates of one being one inch square, and those of the other two inches. The tangents of the deflections of the needle (proper precautions having been taken for the equally free passage of all the electricity evolved in either case,) were very nearly as 1 to 2, though it is obvious that the real quantities of voltaic electricity were as 1 to 4. . The author's torsion galvanometer gave the degrees of torsion nearly as 1 to 4. Other experiments led to similar conclusions.

The author then examines the laws which were supposed to connect the conducting power of a wire for electricity, with its length and diameter, and which, according to Professors Cumming and Barlow, varies directly as the diameter, and inversely as the square root of the length; but, according to MM. Becquerel and Pouillet, directly as the square of the diameter, and inversely as the length. He points out the false conclusions of M. Becquerel, and that he has, in fact, deduced the value of *two* unknown quantities from *one* equation; and that M. Pouillet having arrived at his through the fallacious indications of the common deflecting galvanometer, they are equally erroneous. The author then shows that the law pointed out by Cumming and Barlow is, in ordinary cases, nearest the truth; though, under certain circumstances, the limits of even that law may be passed. Hence, and from a series of experiments with the torsion galvanometer, he arrives at the unexpected conclusion, that there is no determinate law of conduction, either for the length or diameter of the wire, but that it must vary, in every case, with the size of the plates, and the energy of the acid solution used in exciting them. This conclusion the author shows to be in accordance with the views of conduction which he had previously published; namely, that there is no actual transfer of electricity, but that all the phenomena result from the definite arrangement of the electric fluid essentially belonging to the conducting wire.

The second part of this paper relates to certain properties of electro-magnets. No attempt seems to have been hitherto made to investigate the law which connects the lifting power of electro-magnets with their length. The author found, by experiments with two soft iron horse-shoe electro-magnets, to each of which the same short horse-shoe lifter was adapted, and the circuit of one four times that of the other, that their lifting powers were nearly inversely as the square root of their lengths. By increasing the strength of the battery with which they were connected, their lifting powers approached more nearly to a ratio of equality; by diminishing it, the ratio increased in favour of the shorter magnet. Hence the law in this case seems to be as indefinite as in that of common electric induction, and the relation of the powers to vary with the energy of the inducing voltaic influence. By another experiment, the author shows that all that is necessary in preparing a powerful electro-magnet is simply to roll a ribbon of copper about a short bar of soft iron, and to use a short horseshoe lifter of soft iron. The quality of the iron has great influence on

the power of an electro-magnet; and the author found that the worst part of a bar of the worst iron he could procure made by far the best.

A bar electro-magnet, four feet long, which scarcely retained any power when its connexion with the battery was broken, on being *re-connected* with it, in the *same* direction as before, was *rapidly* converted into a powerful magnet; but after being removed, and its wires now connected with the *opposite* poles, it required a long time to convert it into a magnet of much inferior power; as if the atoms of electricity, having been first put in motion in one direction, are afterwards more easily turned in that direction than in the contrary.

The author failed in his attempts to make a permanent horse-shoe magnet of tempered steel by the touch, with an electro-magnet: not the slightest trace of magnetism was communicated to the steel; on the contrary, a previously magnetized horse-shoe magnet had its power completely destroyed by similar means.

Dr. Ritchie describes a method of making an electro-magnet revolve in a horizontal direction about its centre, by permanent magnets properly arranged. This method consists in changing the poles of the soft iron magnet the moment it passes the pole of the steel magnet, so that attraction is almost instantaneously changed into repulsion, and the motion rendered continuous.

In the third part the author describes a mode of obtaining a continuous current of electricity by the induction of common magnets. Any number of soft iron cylinders, having a coil of copper ribbon covered with thin tape wound round the middle, are fixed in such a manner to a revolving table, that they can be brought in rapid succession opposite to the poles of a permanent horse-shoe magnet: the soft iron cylinders are thus converted into temporary magnets. The copper ribbons are so connected, by means of wires soldered to their ends, with well amalgamated discs beneath, that their contact with them is successively made and broken, as often as the soft iron cylinders pass opposite the poles of the permanent magnet: and a delicate galvanometer is made to form part of the circuit. On putting the revolving table into rapid motion, an electric state is induced on the copper ribbon, and consequently on the continuous circuit, from the moment the magnet has begun to act on it till it has acquired its state of greatest magnetic power. The connexion being then broken, by the wire attached to the end of the copper ribbon leaving the amalgamated disc of copper beneath, the needle would return to its former position were it not prevented by the formation of a new current, from the next cylinder of iron coming within the action of the magnet; and, by employing a greater number of magnets, the developement of the fresh current may be effected before the preceding one has been broken off, and the needle be thus made to show a steady deflection.

The author failed in all his attempts to effect chemical decomposition, even of the most easily decomposable compounds, by means of the nearly constant current of electricity produced by his present apparatus; and previous to making a more powerful one, he wished to ascertain whether water be a conductor of electricity thus developed, or not. For this purpose a film of hot water, of more than fifty square inches, was made to form part of the circuit of the magneto-electric

battery; the whole being properly connected with an exceedingly delicate galvanometer. On making the apparatus revolve rapidly, not the slightest deflection of the needle was perceptible. Hence, if so large a surface of hot water be incapable of conducting as much electricity as would agitate the most delicate astatic needle, though the exciting cause was sufficient to make a wire revolve round a magnet, and overcome the resistance of the mercury through which it was dragged, it would require an enormous power of this kind to *decompose* water. The author, therefore, considers it unlikely that electricity induced by magnets will ever supply the place of the voltaic battery in effecting chemical decomposition; and he concludes by observing, that "as no increase of electro-magnetic power is gained by increasing the *decomposing* powers of a battery, and as action and reaction are equal, it appears improbable that we shall ever obtain high decomposing powers by any increase in magneto-electric induction."

A paper was then read, entitled, "Notice of the Remains of the recent Volcano in the Mediterranean." By John Davy, M.D. F.R.S. Assistant Inspector of Army Hospitals.

The author communicates an account given by Captain Swinburne, dated the 24th of August, of a dangerous shoal, in latitude 37° 9′ N. and longitude 12° 43′ E., consisting principally of black sand and stones, with a circular patch of rock, which has been left by the volcano that lately appeared in the Mediterranean. Captain Swinburne furnished the author with two specimens of the air which was seen rising from the site of the volcano, in small silver threads of bubbles. These were found, upon examination by chemical tests, to consist of between 9 and 10 parts of oxygenous to 79 or 80 of azotic gases.

The author adduces arguments in favour of the supposition that this air is disengaged from sea water at the bottom in contact with the loose and probably hot ashes and cinders composing the shoal, rather than that it arises from the extinct volcano. He is also disposed to extend this theory to the explanation of the gases disengaged from hot springs, which are generally found to consist of a mixture of oxygenous and azotic gases, the former being in less proportion than in atmospheric air, in consequence of its abstraction by oxidating processes from the air originally contained in these waters.

The Society then adjourned over the Easter vacation, to meet again on the 18th of April.

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April 18, 1833.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

Thomas Botfield, Esq.; Sir William Burnett, Knt. K.C.H.; Major F. H. Shadwell Clerke, K.H.; Robert Adam Dundas, Esq.; the Rev. Augustus Page Saunders, M.A.; and Thomas Stephens Davies, Esq., were elected Fellows of the Society.

A paper was read, entitled, "On Improvements in the Instruments

and Methods employed in determining the Direction and Intensity of Terrestrial Magnetism." By Samuel Hunter Christie, Esq. M.A. F.R.S.

The tedious nature of the observations by which the direction and intensity of the terrestrial magnetic force are determined, and the uncertainty attending the results when obtained, have long been a subject of regret to all who are engaged in the investigation of the phenomena of terrestrial magnetism. Sensible of this, the author's attention has at different times been turned to the improvement of the instruments employed for these purposes; and in this communication he proposes methods by which he considers that these instruments might be so improved that the results should be obtained with greater facility and also with greater certainty. The uncertainty attending the results obtained with the dipping needle, as at present constructed, arises principally from the two sources, friction upon the axis, and the want of coincidence of the needle's centre of gravity with the axis of motion; the latter rendering necessary the inversion of its poles. The author suggests a method by which he considers that, probably, the friction may be diminished; but he has principally directed his attention to obviate the necessity of the inversion of the poles.

In order to remove the practical difficulty attending the adjustment of the centre of gravity to the axis of motion, an operation in which the artist rarely, if ever, completely succeeds, the author proposes to dispense with this condition; and shows how the dip may then be determined, without the necessity of inverting the poles of the needle, the position of its centre of gravity having been determined previously to its being magnetized. The advantages attending the method proposed by the author are not, however, restricted to the determination of the dip with greater accuracy and greater facility: a further and still greater advantage attending the use of a dipping needle on the principle he proposes, is, that a measure of the terrestrial magnetic intensity will be obtained by the same observations which give the dip; so that, by this means, the observations usually required for that purpose, and which are of the most tedious nature, will be avoided. To effect both these objects in the most convenient manner, he proposes that the needle should be so constructed that its centre of gravity should be out of the axis of motion, in a line perpendicular to that axis and to the axis of the needle. The requisite formulæ for determining the dip and the measure of the terrestrial intensity, in this case and also when the centre of gravity is in any other position, are investigated in the paper. Mayer had previously pointed out that the dip might be determined by means of a needle having its centre of gravity out of the axis of motion, and had given the formulæ requisite for that purpose. His object, however, does not appear to have been the same as our author's,—the avoiding in all cases that source of inaccuracy, the inversion of the poles of the needle,—but simply the determination of the dip, whether the centre of gravity of the needle were made to coincide with the centre of motion, or not: the determination of a measure of the terrestrial intensity, by such means, does not appear to have entered into his contemplation.

As another form in which the same principles might be advantageously applied, the author proposes that two needles, similar in all

respects, should be placed on the same axis; and points out how, by means of such a compound needle, both the dip and intensity might be determined by independent methods, so that the agreement of the results would afford a test of the accuracy of the adjustments and of the observations. He considers that the knife-edge support, which has recently been adapted to a dipping needle, would be peculiarly applicable to a needle of this construction. The sensibility of such a needle would be much greater than that of any hitherto constructed, and the utmost delicacy would be required in the adjustments; but if the needle were accurately constructed, and due care were taken in the magnetizing, and in making the adjustments and observations, the author expects that the dip and intensity would be determined to a degree of certainty hitherto unattained.

The advantages proposed to be derived from the use of a dipping needle on the principle described in this paper, are, that as the dip would be obtained without inversion of the poles, the results would be less liable to error than when that operation is necessary, and the observations would be made in less than half the time usually required; and that a measure of the intensity of terrestrial magnetism would be obtained from the same observations which give the dip, the intensity of the force being thus always determined by means of the same needle, and at the same instant as its direction.

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April 25, 1833.

MARK ISAMBARD BRUNEL, Esq., Vice-President, in the Chair.

A paper was read, entitled, "An Account of an extraordinary luminous appearance in the Heavens, seen at Athboy in Ireland, on the 21st of March, 1833." By the Right Honourable the Earl of Darnley. Communicated by John George Children, Esq. Sec. R.S.

The noble author's house is situated in lat. 53° 37′ N., long. 6° 54′ W. On the evening of the 21st of March last, at 9 P.M., a stream of luminous matter, reaching from the eastern to the western horizon, which it entered to the north of the constellation of Orion, was observed passing about midway between the Great Bear and Arcturus, and directly over the two principal stars of Gemini. The phenomenon was not accompanied by the usual flashings of an Aurora, but appeared to flow, when attentively observed, in a rapid stream from east to west, and varying in intensity in its course. His Lordship compares it to the stream from the pipe of an engine played over the head of a person standing under it, about the middle of its course.

The light was most brilliant at the eastern extremity of the arch, where it was about 1° wide, gradually increasing in width and diminishing in intensity as it approached the western extremity, where it may have occupied about 5° or 6°. Stars of the second and third magnitudes were distinctly visible through the arch, at least from the meridian to the western horizon; and though not apparently at a great elevation, light clouds occasionally seemed to pass between it

and the observer, obscuring its light. During twenty minutes that Lord Darnley observed the phenomenon, it seemed to proceed through its whole extent from north to south, its edges, which, when first observed, extended equally on either side of Castor and Pollux, having in that time entirely left the most northern of those stars. It had wholly disappeared before ten o'clock.

Lord Darnley did not see the beginning of the phenomenon; but was informed that it appeared at first like the moon rising, and gradually extended from the eastern to the opposite horizon. The reflection thrown on the earth was faint: the degree and colour of the light may be compared to that of a comet; of greater brillinncy, however, than any that has appeared in this century.

In a postscript, His Lordship states, that precisely the same appearance was observed at Castlereah, distant sixty miles; and, according to a Carlisle paper, somewhere in the North of England; the time of appearance in both cases corresponding very nearly with that of his own observation.

A paper was also read, entitled, "On the Magnetic Power of Soft Iron." By Mr. Francis Watkins. Communicated by Michael Faraday, Esq. D.C.L. F.R.S.

When free magnetism is developed by induction, and is not retained in that state by what has been termed the coercive force of hard steel, it has generally been considered that all the phenomena due to the existence of free magnetism cease on the removal of the inducing cause. The object of the present communication is to show that such is not the fact. From a variety of experiments described by the author, it appears that soft iron continued to exhibit strongly the attraction due to the developement of magnetism long after the means by which the magnetism had been originally excited had ceased to act. In these experiments, bars of soft iron, in the form of a horse-shoe, had a single helix of copper wire wound round them, so that on the ends of the wire being brought into contact with the poles of a voltaic battery, the iron became an electro-magnet. With one of these horse-shoes, while the connexion between the ends of the helix and the poles of the battery existed, the soft iron, having a keeper applied to its poles, supported 125 pounds; it supported 56 pounds after that connexion had been broken, and continued to retain the power of supporting the same weight after an interval of several days, care having been taken not to disturb, during the time, the contact between the horse-shoe and its keeper. On this contact, however, being broken, nearly the whole attractive power appeared to be immediately lost. The author describes several instances of the same kind, particularly one in which the contact between the ends of the horse-shoe of soft iron and its keeper having been undisturbed during fifteen weeks, the attractive power continued undiminished. Although the interposition of a substance, such as mica or paper, between the ends of the horse-shoe and its keeper necessarily diminished the force of attraction, it did not appear to diminish the power of retaining that force. In a case where the electro-magnet of soft iron and its keeper were equal semi-circles, the author found, what may appear singular,

that the arrangement of the magnetism during the time that the electric current traversed the helix, appeared not to be the same as after the cessation of that current; in the one case similar, and in the other dissimilar, poles being opposed to each other at the opposite extremities of the two semi-circles.

Whether the magnetism was originally developed in the soft iron by means of an electric current passing round it, or by passing over its surface the poles of an electro-magnet, or those of a common magnet of hard steel, it appeared to possess the same power of retaining a large portion of the magnetism thus developed. The retention of the magnetism does not appear to depend upon the relative positions of the ends of the horse-shoe and the keeper remaining undisturbed, but on their contact remaining unbroken: for one keeper was substituted for another without diminution of this power; care being taken that the second should be in good contact with both ends of the horse-shoe before the complete removal of the first.

This power of soft iron to retain the magnetism developed in it was also shown by the action of the ends of the horse-shoe magnets upon a magnetized needle; by the attraction of iron filings; and by the evolution of the electric spark, by means of a suitable apparatus, on the sudden rupture of the contact between the keeper and the horse-shoe, when several days had intervened since the removal of the battery by which the magnetism had been originally developed.

The author's views on entering upon these experiments were, that the soft iron, with its keeper, resembled a closed voltaic circuit; but they have convinced him that the phenomenon of the permanency of the magnetism resolves itself into a case of complex induction, between the soft iron horse-shoe and the keeper.

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May 2, 1833.

HIS ROYAL HIGHNESS THE DUKE OF SUSSEX, K.G., President, in the Chair.

The Right Honourable the Earl of Darnley was elected a Fellow of the Society.

A paper was read, entitled, "Essay towards a first approximation to a Map of Cotidal Lines." By the Rev. William Whewell, M.A. F.R.S. Fellow of Trinity College Cambridge.

The general explanation of the phenomena of the tides originally given by Newton, although assented to by all subsequent philosophers, has never been pursued in all the details of which its results are susceptible, so as to show its bearing on the more special and local phenomena, to connect the actual tides of all the different parts of the world, and to account for their varieties and seeming anomalies. The first scientific attempt that was made to compare the developed theory with any extensive range of observations, was that of Daniel Bernouilli in 1740: the subject has since been pursued by Laplace and Bouvard, and still more recently by Mr. Lubbock. But

the comparison of contemporaneous tides has hitherto been unaccountably neglected: and to this particular branch of the subject the researches of the author are in this paper especially directed; the principal object of his inquiry being to ascertain the position of what may be called *cotidal lines*, that is, lines drawn through all the adjacent parts of the ocean where it is high water at the same time; as, for instance, at a particular hour on a given day. These lines may be considered as representing the summit or ridge of the tide wave existing at that time, and which advances progressively along the sea, bringing high water to every place where it passes. Hence the cotidal lines for successive hours represent the successive positions of the summit of the tide wave, which in the open sea travels round the earth once in twenty-four hours, accompanied by another at twelve hours' distance from it, and both sending branches into the narrower seas. Thus a map of cotidal lines may be constructed, at once exhibiting to the eye the manner and the velocity of all these motions.

Although the observations on the periods of the tides at different places on the coast and different parts of the ocean, which have been at various times recorded, are exceedingly numerous, yet they are unfortunately for the most part too deficient in point of accuracy, or possess too little uniformity of connexion to afford very satisfactory results, or to admit of any extended comparison with theory. With a view to arrive at more correct conclusions, the author begins his inquiry by endeavouring to determine what may be expected to be the forms of the cotidal lines, as deduced from the laws which regulate the motions of water: and he proceeds afterwards to examine what are their real forms, as shown by the comparison of all the tide observations which we at present possess.

The paper is divided into five sections. In the first the author treats of cotidal lines as deduced theoretically from the known laws of the motion of fluids. On the supposition that the whole surface of the globe is covered with water, the cotidal lines would coincide with the meridians, and would revolve round the earth from east to west in something more than twenty-four hours, with a velocity of nearly 1000 miles an hour at the equator. The form and the regularity of these lines would be materially affected by the interposition of land in different parts of this ocean, whether in detached islands, or groups of islands, or large continents, occupying a considerable portion of the surface. In these cases the primary wave will be broken, deflected and variously modified, so as to give rise to secondary or derivative tides, sometimes separating into branches, and producing points of divergence; sometimes uniting at various places, or points of convergence; and at other times producing, by more complex combinations, various phenomena of interference, and other apparently anomalous results. Such is the general character of the tide-waves that actually proceed along the coasts of the Atlantic: and the modifications in their course and velocity are still more perceptible in bays, gulfs, and narrower channels and inlets of the sea, as well as in their progress along rivers. The author traces in detail the effects which these different circumstances may be expected to produce. He adverts to an important distinction which has frequently

been lost sight of, between the progressive motion of the tide-wave and the actual horizontal motion of the water, or tide-current; motions which do not bear any constant relation to one another. Hence the change in the direction of the current does not invariably indicate the rise or fall of the water.

In the second section he examines the causes which have led to inaccuracy in making observations on the tides; the first of which is dependent on the circumstance just mentioned, of the occasional want of correspondence between the times of *high* and of *slack* water; the former referring to the moment of greatest elevation, the latter to that when the direction of the current changes. The other causes of error are derived from the change which takes place in the course of the day in the moon's angular distance from the sun; from the half-monthly inequality in the establishment, arising from the relative position of the sun and moon during each lunation; and from the necessity that exists of making a correction for what may be termed the *age* of the tide; that is, the interval of time which has elapsed between the period of the origin of the wave and the time of its actual arrival at the place of observation.

The third section, which forms the chief bulk of the paper, is occupied by a statement and discussion of the tide observations now extant, and which the author has, with great industry, collected from a variety of sources, both of published accounts, and of manuscript documents preserved in the Admiralty. Commencing with the tide-waves, first of the eastern and then of the western coasts of the Atlantic, he follows them to the Northern sea, and to the different coasts of the British islands, and of the German Ocean. He passes next to the examination of those of the Southern Atlantic at Cape Horn and the adjacent coasts; thence tracing them, as far as the present imperfect data will allow, along the western shores of the American continent, to the central parts of the Pacific, and in their progress across the Australian and Indian Oceans. He likewise examines the condition of the tides in rivers, as to the magnitude and velocity of the undulations, the occasional production of a high and abrupt wave, or *bore*, and as to the influence of the natural stream of the river upon the different periods of elevation or depression of the water.

The fourth section contains general remarks on the course of the tides, suggested by the preceding review of the phenomena they present; on the velocity of the tide-wave; on the form of the cotidal lines; on the currents which attend the tides; on the production of revolving currents; on the magnitude of tides; and on the constancy of the cotidal lines. He adverts also to some peculiarities resulting from interference, such as the differences of the two diurnal tides, and occasionally the occurrence of single day tides.

In the concluding section the author offers various suggestions respecting the most eligible mode of making observations on the tides, and of correctly reducing them when made.